

Expert Advice and Pragmatic Rationality

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Scholars have given advice to decision makers ever since Plato was invited to Dionysius's court in Syracuse in the fourth century B.C.¹ The impact of the advice was never unrelated to the experience and authority of the adviser, but the main determinant was, and perhaps still is, how much trust the advisee put in the adviser.

In the knowledge society, expert advisers base their authority on the scientific basis of their advice. Trust in their advice will now also be related to the overall confidence in science, which may not be high, at least if one takes seriously one of Richard Nixon's aides who said (at the time of the controversy over the Supersonic Transport airplane): "ah, who believes scientists anyway!". Scientific experts themselves are in any case often concerned about their social or public legitimation, and the possibility of lack of trust in science.

What *is* different in the present situation is the public or semi-public character of much of the advice, which introduces different, more universalistic requirements of validity. In this chapter, I want to show that the validity of expert advice in a knowledge society does not, or at least not only, depend on the supposedly universalistic character of scientific knowledge. The situation is more complex, and the expert's role encompasses more considerations than scientific validity alone. In addition, scientific knowledge is not produced in a social and political vacuum, and this is reflected in what we know, and what we do not know, and we want to know.² For scientists, the fact that expert advice is increasingly based on new scientific research, or new assessments of existing research, is a challenge. A new audience requires a different style and a different packaging, and perhaps new substance as well. A striking phenomenon is the external pressure on science to produce "hard" facts. Politicians and other parties in public arenas are not interested in arguments that "on the one hand X, but on the other hand perhaps Y". They get exasperated with such statements in public hearings, and call for a "one-handed scientist".

If available evidence seems to support their position, it is obviously in the interest of decision-makers to make it as solid as possible. Sometimes, however, the interest is in softening the facts: when delays are tactically useful, or when the

position of one's opponent has to be undermined, the uncertainties of science may be emphasized.³ It is true that in our scientific age, it is important for decision-makers to present science as an independent input into the decision-making process, but they will make an effort to have it serve their ends, or at least that it does not counteract them.

Scientists are often happy to oblige, and transform the provisional fabrications of the lab bench into simple, black-and-white facts. But the blackness and whiteness of such facts is the result of a translation process: complexity is reduced with the public arena in mind into which these facts will be delivered. Instead of a scientific forum there is now a hybrid forum: the results should be acceptable to colleague scientists, but also exert some force in socio-political arenas.

The discussion, after Chernobyl, of standards for admissible radiation levels in food and in the environment is a case in point. When lichen in the North of Scandinavia turned out to be heavily contaminated, and the meat of reindeer feeding on it had a radiation level up to ten times as high as the then admissible level, a number of "work-arounds" were proposed to save the meat (and the lifestyle of the Laps). One of them was to raise the standard to ten times its original value, the argument being that this would increase the radiation pressure on humans only negligibly (at least for those who do not eat too much reindeer meat). Scientific calculations went into this argument, while credibility was also important: Norwegian Laps criticized the decision because it applied to reindeer meat only, not the other food. Thus, the public would see it as an ad-hoc and political ploy, and not feel confident about the safety of the reindeer meat. This particular criticism did not get any follow-up, but it could have led to pressure on a whole range of standards and a re-assessment of the evidence for them.

What this brief discussion highlights is the point that fabrication of facts for public arenas has to manage the pressures and constraints deriving from socio-political forces, in addition to the force of experiment and argument. The balance of these forces will create focal points, where effort has to be exerted, whether in assembling and reassessing evidence, or in managing the insertion of fact fabrication in socio-political processes. Thus, the scientific work behind standard setting is not just the limited task of overcoming local uncertainties in solving the problem of a safe concentration level. It also requires assessments of what the problem really is: is it about biological mechanisms of carcinogenesis, or about expected exposure levels? Is the aim to achieve scientific stability with respect to scientific peers, or socio-political stability with respect to interest groups and public debate? In fact, local uncertainties can only be addressed after one has decided what is to be considered certain and what uncertain – and this choice itself is often what is at issue in a public debate. Can we take the negligibility of cancer risks of low-level radiation or minute amounts of dioxin as certain, i.e. not an issue that we have to look into, or should it be considered uncertain and in need of further investigation? Much of the debate in public controversies draws its heatedness, as well as its inconclusiveness from the fact

that the argument is played out on two levels at the same time. Any particular "fact" will be discussed in detail only if it is relevant, i.e. if there is some consensus about the agenda (what is to be considered uncertain and in need of resolution); but at the same time, such "facts" will be submitted as part of an argument to put such an issue on the agenda at all. A highly visible case of cancer, or a dramatic exposure to radiation, with the attendant concern about causality and health effects, will be important in setting the agenda, even when the facts of the matter still have to be fabricated, and some parties argue that existing knowledge suggests that there is no cause for concern.

I. The Ozone Layer Debate

As an example, I shall discuss the case of the possible damage from chlorofluorocarbons (used in spraycans) to the stratospheric ozone layer.⁴ The debate started in the 1970s with an "early warning" by atmospheric scientists, and the two levels were therefore visible from the beginning: is there cause for concern? what is the value of the facts and what further research should be done? The different parties involved had their own research agendas and socio-political agendas, and it is the overall socio-cognitive dynamic of the controversy which determines what the focal points will be, and thus, what kind of research will become important.

In the course of time some procedures and apportioning of roles crystallize and the fabrication of facts becomes semi-public again, as in most cases of standard setting. Before that happens, however, scientists are faced with a lack of boundaries between their work and the public debate, and attempt, after the first heady months of early warning and publicity, to create some closure, if not cognitively, then socially. One of the leading atmospheric scientists said (*New Scientist*, 24 June 1976: Vol. 70, p. 685):

At the moment, half-baked ideas are being produced at a ferocious rate. That's all right when you're only talking to your friends, but it's most regrettable that scientists are telling politicians that they must regulate (as if the evidence was hard).

So, scientists should have refrained from speaking out until they had hammered out some solid solution among themselves. I shall show in more detail how ineffectual such a proposal is, but the above statement shows clearly how scientists felt they had let the djinn out of the box, and now tried to put some box around it again.

Early in 1974, two American atmospheric chemists, Rowland and Molina, speculated about chlorofluorocarbons (cfc), often called "freons" after the Dupont

trade name, and a propellant widely used in spray cans because of their safety and low cost. They were not being decomposed in the lower atmosphere because of their chemical inertia (which made them safe as a propellant), but the amount present in the lower atmosphere, according to sensitive concentration measurements, appeared to be lower than the amount emitted until then. The only place where they could disappear to seemed to be the stratosphere, and there they would be decomposed by hard UV radiation from the sun. The resulting increase in the concentration of reactive chlorine radicals, however, would reduce the stationary concentration of the stratospheric ozone layer. This, in turn, would allow more of the hard UV radiation, normally absorbed by the ozone layer, to reach the earth's surface, causing a higher incidence of skin cancer and possibly other negative effects.

From an article published in *Nature* in June 1974, which did not draw any public reactions, Rowland and Molina went on to a presentation at an American Chemical Society conference in September 1974, the kind of meeting that attracts professional chemists from industry and government as well, and is covered by the media. There, their piece of informed speculation turned into an "early warning". Interested parties started querying the implication Rowland was now drawing: stop using cfc in spraycans. Other atmospheric scientists confirmed their reasoning as potentially correct. Policy-makers, media, and environmental groups took up the cause of the ozone layer. Thus, an arena was created for the main contestants to engage in a scientific-cum-political struggle, and high on the agenda were the questions: Are there sufficient ground for concern? Should we do something, and if so, what?

The main battle was waged during the second half of the 1970s. Although the controversy has not been resolved, the debates have become less public now, the main activity going on between national governments, industry organizations, and international bodies. The decrease of public debate has in fact led to a feeling that there might not be a real threat to the ozone layer, after all. More recently, the discovery of unexpected "ozone holes" above the Antarctic led to renewed public interest. This surge of publicity is, in fact, superimposed on a long-term trend toward recognition of the seriousness of the issue by governments and industry, even while the causality and the extent of impacts is still not completely clarified. Since 1987, there is a framework convention, the Montréal Protocol, for the protection of the ozone layer, signed by many countries (with a recent agreement, signed by about 60 UN members, to reduce cfc emissions to zero by the year 2000): industry spokesmen, at least in the U.S.A., also support some international regulation of cfc.

It is not my intention to retrace the full course of the controversy here. My aim is to analyze how facts are fabricated between contending actors in public arenas, and the case of impact of cfc on the ozone layer is particularly suitable because the science, as well as the socio-political network of it, are inextricably interwoven. The main scientific problem is the complexity of the chemical-

meteorological atmospheric models, the uncertainties about relevant reactions and transport routes, and a lack of data. Present atmospheric models contain about 160 chemical reactions and more than 40 reactive species. Each time a reaction rate is remeasured, a new reaction or new species discovered, or an additional product found for a known reaction, the whole interlocking system is affected, sometimes with dramatic differences in model outcomes. The research delivering such new insights is not conducted in a socio-political vacuum: many of the scientists involved are employees of or funded by industrial or government organizations like Dupont Company, the US Chemical Manufacturers Association, U.S. National Air and Space Administration or the U.S. Environmental Protection Agency. And even without such direct ties, scientists will respond to the state-of-the-debate, as they respond to the state-of-the-art in more academic scientific fields. Thus, the input of new research results into the models, and consequently, outcomes as well, are not independent of the perspectives and interests of the actors.

From the beginning of the debate, in the autumn of 1974, three kinds of actors could be distinguished. There are the chemical industry and the aerosol manufacturers, who (in the words of Dupont Company's nationwide public relations campaign of the time) want both themselves and the ozone layer to survive. There are environmentalists, concerned scientists, and worried consumers, who each from their own background and goals, argue that the ozone layer should be protected from unnecessary interference. There is a third actor, US federal agencies and State governments seeing an opportunity to expand their regulatory scope, Congressional committees fighting for "turf", and national and international government bodies everywhere whose business it is to negotiate and regulate, serving the public interest at the same time. Researchers are linked to one of these three kinds of actors, directly or because they share the goals. Sometimes they try to maintain a quasi-independent position as well, in order to avoid contaminating their science, as well as the standing of their input in the debate.

Although there were, at the beginning, many uncertainties, lack of data and lacunae in understanding, it was not just because science did not deliver a conclusive solution, and left an "open space" in the arena, that there was room for interests and values of the actors to influence their positions and actions. The different actors defined the cognitive situation itself differently.

For industry, there was only a speculative "theory" (as they called it pejoratively), and no regulation should occur until research had been conducted to clear up the basis for this "theory". Accordingly, their own research focused on measurements of actual concentrations, and on alternative "sinks" for cfc in the lower atmosphere. If cfc would, for instance, decompose in sunlight at the surface of sand grains – amply available in the Sahara and other deserts – that would explain away the concentration deficit in the lower atmosphere, and have the added benefit of making the difficult measurement of chlorine concentrations in

the stratosphere superfluous. Some effects of sunlight and sand were indeed found, although too small in magnitude to explain the deficit. But as a result, much more became known about this particular part of the problem: its certainties and uncertainties were well articulated.

The government interest in a solid basis for regulation led them to support the construction of models for stratospheric chemistry and meteorology that would allow predictions of future ozone layer concentrations under different scenarios of cfc emission, and thus provide guidelines for regulation. Within this overall goal, the US government agencies focused on one-dimensional models (i.e. considering vertical transport only, no horizontal transport) instead of more realistic two-dimensional models. In this way, different model structures and outcomes could be compared and a consensus could hopefully be developed on how to regulate. Other governments, in Britain or Scandinavia, with less interest in immediate action, saw their task as creating understanding of the issue, and supported two-dimensional modelling. In retrospect, the one-dimensional models are now assessed as superb to derive "what if" scenarios, but difficult to relate to the actual world, where the atmosphere is not the same over the equator and the poles.

Environmentalists were often less concerned with research than with arguing how unwise it would be to run even a small and speculative risk of disturbing the atmospheric balance. Their problem definition for scapegoating cfc can be recognized in the tenacity with which atmospheric scientists like Rowland, who invested heavily in pushing the early warning about cfc, focus their research on cfc as causal agents for atmospheric phenomena, including a conviction that the recent Antarctic "ozone hole" is also caused by cfc. Note that this is not a criticism: tenacity in pursuing a line of research is considered a scientific virtue. The point is, rather, that tenacity can be maintained only when it is embedded in a supporting network, in this case one linked to an early warning/environmental critic role.

Not only are research agendas set and followed by the problem definitions of the different kind of actors, the agendas and problem definitions interact and create focal points for debate and research. Such focal points then become much more articulated than other parts of the problem. An example is how the first scientific report on the ozone layer problem, produced by the U.S. National Academy of Sciences, and invested with a lot of weight by all actors, had to be delayed when in the spring of 1976, at a late stage of the writing, a new finding was reported. It appeared (from research by Rowland) that two of the chemical species involved in the chain leading to reduction of the stationary ozone concentration, could react and form a compound that was much more stable than originally thought. Since such a compound would provide an additional stratospheric "sink" for cfc's reactive products, there would be less damage to the ozone layer. It was clear to everybody that this would have to be studied in detail before the report could be released. All the actors were waiting for the report, and would jump on this possibility to deconstruct it, or solidify its facticity, depending

on their problem definitions. The effect was that predicted decreases of the ozone layer (for the 21st century) were modified downwards, but still large enough for some government agencies to start preparing regulation, although the National Academy itself had advised to wait for two years' further research into the issue.

It will be clear that the state-of-knowledge at any one point of time, that is, the map of understanding of the problem upon which advisory reports and actor's stances are based, is not independent of the previous evolution of the controversy. Problem definitions of powerful actors, as well as the evolving agenda of the debate, determine where the topography is more or less detailed or shows white areas of *terra incognita*. Socio-political actors and interactions thus help to determine what is known and what is problematic. In fact, it is impossible to disentangle "value" and "fact", and it is of little use to strive after it. It will not contribute to the resolution of the controversy, even if it may create some division of labour between scientists and other actors.

To complete the analysis, the same line of argument can also be applied to the state of knowledge at the time Rowland and Molina started thinking about cfc and the ozone layer. Stratospheric chemistry of that time was shaped by the concern to know about the impact of exhaust gases from rockets and spacecraft, and more recently by the debate about the impact of the proposed supersonic transport aircraft. Accordingly, the U.S. National Air and Space Administration was the biggest funder and assessor of research, and interest in, as well as knowledge of, the impact of nitrogen oxides and dust was highest. Rowland and Molina could base their analysis on a partial understanding of stratospheric chemistry, and had, of necessity, to be speculative to round off their argument.⁵

While I have discussed one historical case only, the point is clear that the dynamics of controversies are irreducibly socio-cognitive, with focal points thrown up through interlocking actors' strategies, which then determine research agendas and the state of knowledge. The present debates on the greenhouse effect, and on global climate changes in general, offer many supporting examples. One striking point is the political mobilization, in the late 1980s, to do (and be seen to be doing) something about global climate change, while the possibility of a greenhouse effect had been raised by scientists since Arrhenius in the 19th century, and some debate and some research had been going on in the 1960s and 1970s. The present massive research effort derives from a political focal point and growing political consensus, so there is no linear causality from scientific expertise to political decision making. In fact, in symposia and editorials in scientific magazines, scientists are urged to create a consensus in order to put some weight behind the political necessity of doing something about global climate change.⁶

II. Standards for Dioxin in the Netherlands

In situations of hybrid forums, as discussed in the preceding section, it becomes vacuous to inquire where science ends and politics begins. Attempts by scientists or others to create or maintain boundaries between the two, although sometimes useful in terms of a division of labour, will also hide the actual dynamics of hybrid fabrication of fact. That might be too high a price to pay for a superficially sound division of labour.

This is a strong claim, and to support it I shall discuss a case of expert advice for a standard for dioxin in the environment. The story of the original standard, its modification, and a recent debate following the discovery of emissions of dioxin from waste incineration plants (in the Netherlands) show that experts actually mix science and politics by following a pragmatic rationality, taking into account the societal effects of various possible standards. This should not be frowned upon: such are the actual dynamics of expertise, and philosophically, it is better to accept it, recognize it, and introduce some accountability, than to push it out of sight. I will come back to this philosophical point, and its relevance for the knowledge society, in the concluding section.

The story starts in 1980. Within the space of a few months, two widely divergent reports on admissible levels of dioxin in the environment were produced, and by the same scientists of the Netherlands Institute of Public Health (a research institute financed by the Ministry of Welfare, Public Health, and Culture, and the Ministry of Housing, Physical Planning and Environmental Management). The first report was written because cancer cases were reported in a rural village, and spraying of the herbicide 2,4,5-T, with dioxin as a contaminant, in the surrounding forests was mentioned as a possible cause. A literature review was made, and the scientists concluded that dioxin should be treated as a proven carcinogen in animal experiments, and one with genotoxic effects. Since the contamination existed already, a scheme established by the Health Council, the main scientific advisory council to the Dutch government on health issues, was followed to calculate the level corresponding to an acceptable risk criterion.⁷ An acceptable daily exposure level of 13 picogram was the result, and the report concluded that there was no real hazard, because measured exposure levels were below 13 picogram. When interviewed later, the scientists involved emphasized that the concentration of 13 picogram was not intended as a standard, only as a reference point to argue that in the actual situation cancers due to accidental exposure to dioxin containing 2,4,5-T were highly improbable – a subtle point, but of importance for later debates.

At the same time, a controversy over a chemical waste dump near Amsterdam came to a head. Wastes of a 2,4,5-T chemical plant had been dumped in the municipal waste dump. After prolonged action by the citizen committee, the

municipal council had to admit that the wastes contained dioxin. The debate then centred on the suspected carcinogenicity of dioxin. An estimated daily intake of 100 to 150 picogram was accepted as a realistic estimate by both parties in the conflict. The citizen group then quoted the report of the Netherlands Institute of Public Health to argue that dioxin was a complete carcinogen, and that an acceptable intake level was 13 picogram, which would require a major and very costly clean-up operation. The experts of the municipal council of Amsterdam quoted an acceptable level of 140 picogram, supporting it by an argument that dioxin was not a complete carcinogen. It turned out that their argument was based on a confidential report from the same Netherlands Institute of Public Health.

Had the scientists turned around completely in the course of three months? One reading of the events is that they had continued their assessment of the evidence after bringing out their original report, and had introduced revisions which had to go through peer review, by members of the standing Health Council committee on carcinogenicity of chemicals, before they could be made public as providing the basis for a standard. (The report was made public two years later.) Another reading, favoured by the citizen groups and their counter-experts, was that the scientists of the Institute were swayed by the daunting prospect of being responsible for a major clean-up operation, and decided to fiddle with the figures. The actual activities of the scientists appear to lie somewhere in between.

No research results were used in the second report that had not been available at the moment of writing the first report. The latter, however, took existing, i.e. published, knowledge for granted, and decided to err on the side of safety by using the possibility of genotoxicity to reach and estimate the risk. When the debate about the waste dump focused on dioxin, however, it became crucial how dioxin should be classified in the Health Council scheme: as a complete carcinogen or only as a promoter. The Institute scientists decided that the mutagenicity tests, on which the suspicion of genotoxicity was based, had to be rejected formally because of their early date, and concomitant looseness of procedure. They also contacted the authors to inquire whether the results had been replicated, which turned out not to be the case.

While in the first report the scientific rhetoric was geared to presenting all the evidence as "hard" as possible, as seemed suitable to the occasion, in the second report the reverse occurred. The "hardness" now resided in statements about the situation, with exposure levels ten times as high as the previously established "standard" being taken as certain. To manage these new constraints, the assessment of evidence was modified, first going back to the original experimental articles, and then questioning the facticity of the experiments themselves. If the facts putting dioxin into the category of a complete carcinogen had been more stubborn, the outcome might have been different. Now, it took relatively little effort to shift dioxin to the other category of promotor only.

The shift did result in further controversy, and in loss of confidence in authorities with the citizen groups, also because the report was kept confidential

during the period of peer review required to create a standard according to the Health Council scheme. Scientific quality control and boundary maintenance reduce instead of enhance social confidence in this case! The external criticisms were insufficient to deconstruct the proposed standard, partly because the network of evidence and argument was hard to rearrange, partly because the claimed separation between scientific and socio-political assessment was accepted by all parties. Thus, there is a curious ambivalence in the role of science in standard setting. On the one hand, it is common practice, and indeed unavoidable, to take socio-political context into account in deciding what to consider "hard", i.e. not suitable for deconstruction.⁸ On the other hand, it is important for scientists to (re-)emphasize their role as expert and make it unassailable by erecting boundaries between their work and public debate.

In their own practice of constructing advice, scientists apply what Ezrahi (1980) has called "pragmatic rationality": within certain limits, scientific criteria may be offset by administrative or political criteria, in order to create a robust outcome. Ezrahi's example is the Consumer (or Retail) Price Index, which has an important symbolic and administrative role in economic policy and negotiations. Its credibility rests, in part, on its acceptance by all parties as a reliable and unchanging reference point. Improving its scientific basis, with attendant recalculations, would only undermine its authority, with very little real gain.

Ezrahi contrasts pragmatic rationality with utopian rationality, where consensual science offers authoritative advice to a political arena which docilely follows the advice. This contrast between pragmatic and utopian rationality actually surfaced in the third phase of the dioxin story, when dioxin emissions of waste incineration plants were recognized and measured, and sale of milk from contaminated areas was banned. Farmers, and directors of incineration plants, pressed for relaxation of standards. Toxicologists, on the other hand, argued that the existing standard was already too liberal.

The utopian rationalists have science at their side,⁹ it seems, when they criticize the use of toxic equivalence factors (TEF) for the 17 or so species of dioxins that actually occur (of the 75 theoretically possible), which enable standard setters to calculate the equivalent concentration in terms of the well-known (and assumedly most toxic) 2,3,7,8-tetrachlorodioxin (TCDD), for any actual mixture of dioxins. There is no proof that the safety factor of 250 used for the TCDD no-effect level is applicable to the other dioxins: these may be cancer initiators rather than promoters. The utopians argue that a false sense of security is now created with the public.

In contrast, the RIVM expert is pragmatic:

The TEF concept is a reasonable attempt. While there are uncertainties, e.g., about initiation, there is a structure-chemistry argument to make plausible that none of them is an initiator. Indeed, in principle one should exclude this possibility on the basis of toxicologi-

cal studies. But there are so many other things that have to be studied as well, and one should choose one's priorities.

Van der Heijden refers to a Swiss study of 1985 which showed that some mixtures are less toxic than calculated on the basis of TEF to argue that TEF-values may well be too high.

But these TEF values have not been determined scientifically, they are based on international agreements, i.e. arrangements to be able to *do* something. The TEF-values have been purposively chosen to be on the high side. So to reproach us that the RIVM-standard is a semblance of security only, is a bit demagogical. As if we were swindling the public with this standard. That is definitely not the case.

There are other arguments, for example about the neglect of special situations (like dioxin in mother's milk), and the issue of interaction effects, especially with polychlorinated biphenyls, to which the RIVM expert replied equally pragmatically:

When looking for interaction effects, why stop at PCBs? Why not look at PAC as well? Or nitroso-amines? There is no end to it. Of course, we have *research* going on on interaction effects, but for standard setting we do not do anything with it, for the time being.¹⁰

For pragmatic rationality, the issue of safety factors in standard setting is also very interesting. The interviewer, at one point, asked:

At the time, a safety factor of 250, instead of the more usual 100 was used because of some minor uncertainties/indications. So if 100 had been used in 1982, there would now have been no dioxin affair in the Netherlands?

Van der Heijden:

Indeed, it can be put that way. During last weeks, pressure has been exerted on us, from different sides, to revise the standard. But we won't do that. We have determined the standard in 1982 [i.e. in this context, the safety factor], and are not going to modify it retrospectively. That is to say, we are prepared to change the standard, but only on the basis of toxicological considerations. And not because we are so close to the standard in reality. There are a number of parties for whom it would be convenient if the safety factor had been 100, as Switzerland and Canada have done, instead of 250. But if we would relax that standard under pressure, we would lose our credibility. If one would force us to do it somehow, we would do better to quit our job.

The interviewer then asked why a safety factor of 500 was not chosen at the time. Van der Heijden:

If we had taken over the American standard at that time, that would have implied that the

Netherlands should be treated as being a waste dump; in theory, we could not have lived in the Netherlands then.

The pragmatic rationalism of Van der Heijden, the expert from RIVM, stands out again, and while he may not have science on his side, he certainly has good reasons. He is mixing science and politics, and it is clear that in doing so he tries to answer three types of questions:

- Which research, however important, will not be done?
- How can credibility be maintained in the long term?
- What is the societal effect of setting this standard, and is this acceptable?

It is not easy to align the heterogeneous considerations, but it is the responsibility of the expert, when preparing his advice, to come up with reasonable, and concrete, answers to such questions.

III. Conclusion

Running through the presentation of the two case studies is the notion that choices are being made what to consider certain, what to explore further, and which results to attempt to deconstruct. Such choices are related to the agenda of public or semi-public debates, and to actor's strategies in influencing the debate. Often, focal points emerge around which analysis and debate, and therefore also research, cluster. What we know in a certain domain is based on the research efforts that actors have exerted. In short, the state-of-the-art (i.e. our knowledge) in a domain derives from the state-of-the-debate.

These processes come to a head when explicit advice is expected and prepared. The NAS report in 1976 on the risks to the ozone layer is a case in point, and standard setting for dioxin another. Experts, in constructing their advice, take actual and desired context into account. This is what they actually do, and for that reason alone one can elevate their practice to a prescription: experts should be pragmatically rational, and produce robust advice, which is able to create desired societal effects (or withstand undesirable interference), rather than formally correct statements which have no (or the wrong) purpose. The argument to shift from the descriptive to the prescriptive is thus that the mixing of science and politics, if it occurs anyway, had better be recognized for what it is, rather than being suppressed by assertions about the separation of science and politics, in an attempt to create or maintain special authority for science.

Why is such special authority necessary (apart from the psychological needs of